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# Econometrics: Ghosts of Economic's Past and Tidings of Good Cheer?

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# Econometrics: Ghosts of economic's past and tidings of good cheer?

**By Khorben Boyer**

Econometrics is a field in which the tools of mathematics, statistics, statistical modeling, and computer science are applied to economic data in order to discover patterns, clarify relationships, and discern the veracity of economic theory. It involves the quantitative analysis of any given economic phenomena with the aim providing the empirical grounding for economic's various laws, relations and hypotheses. On an abstract level, this tightens the fruitful spiral formed between the development of theory and the collection of observations by testing various hypotheses. On a practical level, this often requires the creation of what are known as econometric models which are specifically designed to have parameters that can be estimated or determined from the right assumptions and sufficient data. With respect to economics, it arguably provides the vital interface between academic knowledge and the working economy.

## Econometrics's past

Despite this impressive scope, econometrics has not always been as successful as desired by its proponents and admirers. At times, it was not even respected by economists during different periods of developments in economics. While its history stretches back to the 19th century, the field became far more established in the early 1900s. In particular, the field's development became especially important after the Great depression when determining the state of the economy and its critical macroeconomic indicators such as Gross Domestic Product became paramount. In this era, one critical developer of econometric theory was the economist Trygve Haavelmo whose work consisted of attempting to introduce the proper statistical inference techniques and methodology previously established by the great statisticians R.A Fisher, J. Neyman, and E.S. Pearson to the discipline of economics. In particular, an old recovered paper called "The Nature and Logic of econometric inference: the 1942 Hillside Lecture" describes a seminar given by Haavelmo in which he introduced his new viewpoint. In the paper, he argues contrary to the objections made by some economists who were against the practice of statistical inference and of applying its methods within areas of economic research.

His arguments answered several questions of basic methodology starting with whether it was even worthwhile or wise to treat available economic data in the form of stochastic variables. His answer was a positive one though he admitted that statisticians themselves were often of little help as they considered economic problems to be inexactly and even unscientifically formulated questions. Next, he elaborates on his views of econometric research and why probability theory along with statistics is the only sensible solution. Namely, an econometric researcher seeks to direct their attention to specified objects with particular properties which have associated numerical indicators whose behavior should be analyzed. That is when dealing with an observed economic phenomenon or phenomena, one aims to discover the causal mechanism behind it. Once found, it can then be used to repeat and predict the processes' outcomes in the form of new measurements as if they effectively represented the outcomes of an experiment. Critically, the nature of inference involves either attempting to explain seemingly different sets of measurements with the same mechanism or alternatively employing different mechanisms for the same set of measurements. This means that there will be no guarantee that the supposed explanatory mechanism will be persistent. In other words, there will always be some degree of risk that the inferred mechanism is not the true mechanism and that the real underlying mechanism remains unknown. Since the idea of a causal mechanism can be considered abstractly as an observation producing process, any possible set of observations can be potentially described by some given probability distribution over possible outcomes. Thus, the whole of probability theory or its calculus can be used to keep one from making unnecessary errors when attempting to deduce the various consequences of assumptions concerning data. Also, it enables one to determine when a statement is almost certainly true as well as enabling generalization by inference beyond what the collected observational data suggests.

Next, he examines the reservations of economists toward using such a methodology on time series data which is a vital source of economic data. First, he notes that is not required for such a series to consist entirely of independent observations with no dependencies to apply the methods of statistical inference. Second, he identifies what he considers to be the actual problem with time series data. Specifically, it is the fact that such data are just passive observations of the ongoing economic system. A system whose many concurrent processes are difficult to untangle from one another due to their various shared relations. For instance, whether “spurious results” are economically significant depends our theoretical interpretation of the data without which one risks thinking a mere correlation implies a causal relationship where none actually exists or is of a different form. Furthermore, he demonstrates that the behavioral relations needed to enable causal inferences for such examples as policy outcomes can in fact be found from these passive observations of the system; however, it requires careful examination of the consequences of the relations in order to thoroughly test our hypotheses. Hence, well-trained economists will remain vital to establishing what is economically meaningful or not in such data. Thus, we may simply fail to infer what we need from such data to test our hypotheses without additional insights or investigative means beyond what passive observation alone can give us.

Ultimately, his work gave rise to a rapidly developing research program which after only a few years of giving this lecture began to test the limits of technology and knowledge of the time with attempts of using desk calculators to perform full-information maximum likelihood estimates.

## A jump forward in econometrics

Certain aspects of this rapid development of Haavelmo’s probabilistic modeling program by other economists along with others are portrayed in Christopher A. Sims’ paper “Statistical Modeling of Monetary Policy and its effects”. In the article, the author describes a narrative involving how the understanding of monetary policy along with its effects underwent a rocky but upward improvement starting from circa 1950. Key to the narrative is the connection between the field of modeling and statistical inference methods regarding economic time series data and theoretical work on macroeconomic policy effects toward fluctuations in business cycles. The latter field had its foundations in John Maynard Keynes's seminal work on the Great Depression concerning unemployment and its resultant favoring of fiscal policy over monetary policy from his analysis. The former area of econometric methodology was just beginning to employ multiple-equation, statistically estimated time series models as introduced by a Jan Tinbergen in the field. It received significant criticism from Keynes regarding whether it could enable proper testing of economic theories. At the time, it is noted that Haavelmo was aware of these criticisms but nonetheless pointed out the need for such a statistical approach due to the inherent error and imprecision that arises in economic models. Only by casting them into probabilistic form could these models become open to the statistical techniques of hypothesis testing and the possibility of empirical verification. In his own simultaneous equations modeling, he described the possibility of inferring the effects of interventions in these models through simple equation replacements or alterations. This methodology would eventually be known as the area of structural equation modeling and serve a key tool in economics.

**Keynesian Econometrics versus Monetarism**

Moving forward to the mid 1960s, economists had extended this strategy in the process creating Keynesian econometric models. Models that involved over a hundred estimated equations constructed by dozens of model builders over years of effort in order to capture the perceived complexity of the economy even as it proved difficult to properly analyze and test these models as research continued. Furthermore, the members of the monetarism movement such as Milton Friedman were devising mere single-equation regressions to provide support to their view that the economic system did not in fact require such large models to encompass its behavior. Specifically, they considered business cycles to be primarily driven by monetary policy which was considered to be only one of many drivers in the Keynesian models. This divide in methodology and viewpoints of these two groups occurred as neither of them were fully following Haavelmo's recommendation to examine the statistical fit of probabilistically configured theories against each other. Their respective approaches were simply too different. At this point, the monetarists were convinced that stable monetary policy was the key to minimizing cyclical fluctuations whereas the Keynesian econometricians remained skeptical but unable to offer an effective rebuttal. They had used far too many questionable assumptions for the sake of expediency in their complex models. In addition, neither side appreciated the need to include explicit models of policy behavior within their respective models of the overall economy which left them and especially the Keynesians blind to the role of monetary policy fluctuations in business cycles.

**II. What Was Missing**

The paper further elucidates the causes of this impasse by examining two critical weaknesses that had existed in Haavelmo's approach which the econometric modelers had unwittingly foundered upon. First, he had attempted to rely on the Neyman-Pearson frequentist interpretation of statistical inference. This technically requires the parameters of models to not be assigned any probability distribution as they are considered to be nonrandom in nature. This means that the situations involving decision-making with uncertainty could not be effectively analyzed as past decisions are considered to be random in nature with present decisions considered as samples of a given probability distribution which conflicts sharply with the decision maker's own perspective concerning their choices. It also meant that model information could not be easily integrated with the decision maker's own beliefs or knowledge concerning the system in question. This was really only solved later by using a Bayesian interpretation of statistical inference which does allow probability distributions be taken over parameters thereby explicitly modeling one's uncertainty regarding them and enabling the assimilation of model uncertainty with the decision maker's own informative beliefs. Without this change, any attempt to apply the Haavelmo's concepts to the truly macroeconomic sized models being developed would fail to proceed very far as its frequentist hypothesis-testing framework would not scale up to match.

Second, Haavelmo made no attempt to introduce policy behavior equations into his uncomplicated Keynesian models without which no real estimation of the effects of policy could be made. Instead, this neglected feature would end up as a persistent error repeated by a whole generation of economists. As the Keynesian econometric models increased even further in size and complexity, one had to estimate possibly even thousands of parameters using data that was all too insufficient to the task. And where the data's influence ended the rest was filled in using the previously mentioned assumptions. Then, it was evaluated using the Cowles Foundation methodology which had been derived from Haavelmo's ideas without any real attempt to overcome the deficiencies in the methods. In the end, all of these problems reduced the credibility the simultaneous equations research program to the extent that economists began to abandon it.

**III. New Evidence and New Modeling Ideas**

As the Keynesians and monetarists dealt with their own problems, some economists began to use the single equation regression technique to explore other relations in the economy that seemed to contradict the conclusions of the monetarists. To get at the heart of the matter, multiple-equation models were created known as vector auto-regressions or VARs whose developers include the author of the paper himself. Sims states that VARs are restricted probabilistic models of the joint behavior of a set of associated time series involving symmetric probability distributions. These new models seemed to show that interest rates were also main drivers of changes in money stock and hence growth. This in turn challenged the view that variations in monetary policy were the key elements to controlling business cycle fluctuations.

Alongside this progress, macroeconomic models in the 1970's began to be constructed according to the rational expectations hypothesis in which the future expectations within embedded behavioral models were extracted from the greater model's structure with all of its dynamics at play. These resulting models did not offer much support for the work of the monetarists or Keynesians at the time. Furthermore, the inclusion of the hypothesis demonstrated that policy behavior in the private sector had to be modeled if one were to have accurately modeled expectations overall. This posed a problem for the extensively developed Keynesian models of the time. They all involved no modeling of policy behavior since they had instead treated such behavior as external to the model. Since this practice was engrained to such an extent in the Keynesian modeling tradition, the criticism offered by the rational expectations hypothesis was very difficult for Keynesians to adjust to for policy focused economic inference. Indeed, over the course of the next several decades of model building, attempting to create an economically relevant Keynesian model that included rational expectations could not even be estimated at the time. Worse, the lack of such expectations in their standard models made them worthless for policy makers wishing to forecast the various possible futures the economy was envisioned to take based on select variables such as those present in the Phillips curve model. The mere suggestion that the use of such incorrectly predicting models for policy decision making could have been responsible for the 1970s U.S. inflation rate and the related stagflation phenomena proved enough to drastically reduce the interest of academic economists in econometric policy modeling. Despite this dismal suggestion being found to have no real empirical ground, the damage was done to the research interests of the majority of economists of the time often leaving monetary policy institutions to fend for themselves. In addition, the aforementioned VARs models could not replace the loss of these large models for policy as they were purely descriptive of time series data and could not be used to predict the effect of policy choices on the future economy. It was only in the late 1980s that it became possible to formulate structural VARs or SVARs that implemented the kinds of explicit quantitative theory restrictions necessary to make them predictive of policy effects.

**IV. Consensus on the Effects of Monetary Policy**

With time and improvements in modeling, it become clear by 1980 that the interest rate did in fact play a key role in determining money stock thus making money growth a poor indicator of the effects of monetary policy. SVAR minimalist identification proved effective in helping to form this consensus; however, their strict limitations arising from a “black box” system treatment of the economy outside of their isolation of monetary policy made forming conditional forecasts very difficult as opposed to what the contemporary large scale models of the time could provide. Eventually, the proper successors to the Keynesian models arrived in the development of the complete dynamic stochastic general equilibrium model or DSGE in 2005 whose operation reflected what was found using SVAR models for monetary policy. In addition, all components of the model possessed proper economic interpretations with no use of black boxing. Furthermore, the model’s parameters could be estimated and uncertainty regarding them quantified by application of formal Bayesian inference techniques with their date fit being comparable to standard VARs models. Thus, a half a century after the beginning of Haavelmo’s research program, it had finally born fruit in the form of an analyzable macroeconomic model suitable for policy examination that was also cast as a full probability distribution with respect to data. They supported model fitness comparison among themselves along with being able to evaluate the effects of any improvement to the model throughout the entire system with no single equation restrictions. They have been increasingly used by central banks for these properties as before they had lacked such complete support for policy evaluation for decades.

**V. Are We There Yet?**

Nonetheless, the DSGE models are only the first step in accomplishing Haavelmo's project in making economic models that are both robust and testable. For instance, such models despite their sophistication were not able to forewarn us of the 2007-2008 financial crash event and the ongoing recession that has followed it. While probabilistic models by nature will always feature some measure of error, the difference between the models' predictions and the actual economic occurrences shows a degree of error that indicates that significant technical improvement is still needed. Now, it could turn out that such extreme events are like massive earthquakes in being what we wish to predict the most while actually being what we are able to predict the least. To this end, DSGE models are currently including more financial sector activities and the various components of fiscal dynamics to increase their accuracy concerning policy. Moreover, most DSGEs have microeconomic foundations in the form of the constraints imposed on the optimizing agents embedded in the model that are considered empirically questionable and in need for a complete re-evaluation from the ground up. Also, current versions do not possess enough parameters to achieve a better fit as their current number of parameters are far below that of the 1960's and 1970's Keynesian models. Nonetheless, they represent a significant achievement of some of Haavelmo's goals for scientific econometrics.

## Modern Econometrics

Now what impacts these past and current developments in econometrics have had on macroeconomics can be seen Michael WoodFord's paper "Convergence in Macroeconomics: Elements of the New Synthesis". Here the article discusses the advances in various areas of economics and a seeming decrease in critical disagreements regarding various issues. The author notes that throughout the time span from 1960 to 1980 even basic methodology was a source of contention traversing from modeling theory to empirical verification. In particular, the earlier part of this period was defined by the deep divide that separated the neo-Keynesians and monetarists by virtue of their fundamentally different views of how economies worked. Both were interested in predicting the effects of different government policies but the Keynesians were particularly focused on evaluating short-term effects using structural econometric models with estimation in order to achieve good empirical fitting of their models to aggregate time series data. In contrast, the monetarists considered this approach foolhardy and focused exclusively on long-run behavior of the economy where they believed economic theory would produce more reliable and consistent predictions. Moreover, they depended heavily on plotting the relevant changes in the aggregate time series for analysis and treated economic shifts using narrative explanations. Yet by the later part of this period from the 1970s to the 1980s, the new battles lines had been redrawn with the emerging New Classical economist group and the real business cycle theorists who both considered the development of structural models of short-run fluctuations to be the goal of the macroeconomics discipline. This was quite a shift as the differences between this new group and the Keynesians were now centered on merely what each group required for rigor in their macroeconomic models. In particular, they argued about whether theoretical consistency or empirical adequacy should be favored. If the former as it was with the New classical and real business economists, then econometric testing was not a high priority; whereas the latter involving the Keynesians demanded econometric testing.  Overall, Woodford considers the period from the late 1990's on to have had fewer of these sharp contrasts of opinion not simply because the economic field has moved on as it did previously but also due to improvements in macroeconomic analysis and econometrics that have enabled more alternatives to be evaluated in a much more thorough manner than in the past. Specifically, he credits the arising of a new synthesis known as the "New Neoclassical Synthesis" with its integration of the key strands of thought that each school had practiced to have effectively ended the conflicts over method that have defined the field for so long.

**I. Elements of the New Synthesis**

The paper then covers some of the agreements that have arisen in the recent decades. One is that models with groundings in coherent intertemporal general-equilibrum are now considered a must in macroeconomic analysis. The mainstream methodologies employed now owe much to the stances of the New Classical school and the real business cycle theorists. Structural modeling of short-run aggregate dynamics as desired by the Keynesians remains despite past harsh criticisms from the older groups and is now highly respected in the form of dynamic general equilibrium models. Furthermore, these models have been improved to the point of reasonably mapping to short and long run economic responses to economic shifts. Indeed, general equilibrium models are now the preferred type of macroeconomic model with DSGEs used to analyze extremely complex effects arising from the interaction of different policies, imperfect competition, labor markets, and product markets. Econometrically validated structural models are widely appreciated as the foundation to quantitative policy analysis having kept up to date from the 1970s due to significant technical improvements. The ambitions of the original Keynesian modelers are being revisited with worthwhile contributions to macroeconomic theory that are required to serve the aims of achieving a "quantitative theory". In addition, vastly greater amounts of available computational resources enable the specification of relations in structural models to be determined to a far greater extent by how well they fit the data as opposed to fitting the computer's capacity. While some divisions may continue to exist concerning empirical methodology, the aforementioned "quantitative theory" is demanded by all estimators of DSGEs. Furthermore, explicit modeling of rational expectation is considered an important priority by most modelers as a welcome outcome from the Lucas critique from the 1970's. Most agree that inflation can be controlled by proper application of monetary policy.

**II. Remaining Disagreement**

Most remaining disagreements consists of skepticism of whether real economic progress has been achieved, demands for even greater scientific rigor, and questions as to whether the new empirical DSGE models are as truly empirical as claimed. Models have been improved overall throughout economics and by no means have all economic questions or problems been solved. Some even criticize the recent empirical bent of macroeconomics as being too scientific with not enough emphasis on an engineering perspective toward simply solving practical economic problems. It is also noted that less disagreement in economics does not mean that everyone is satisfied with current understanding of various areas of economics. Ultimately, the author ends the paper on a positive note with the macroeconomic field being open for continued progress and improvements brought to the relation between theory, empirical work, and working practice by further developments in econometrics.

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